mechanics, but no longer profess to be able to describe, detail by detail, the ultimate moving elements and motions that underlie these phenomena.

In the second part of his book the author seeks to show that the salient divergences between the schools simply mask the essential congruity of their views. All physicists admit—in whatever idiom they may describe them—the same ultimate objective data; while even if their hypotheses are only methodological instruments of organisation and discovery, it must be recognised that the science presents in the different schools a real though not obvious unity of development.   T. P. N.

How to tell the Birds from the Flowers: a Manual of Flornithology for Beginners. Verses and illustrations. By Prof. R. W. Wood. Pp. 28. (San Francisco and New York: Paul Elder and Company, n.d.) Price 50 cents net, or in cat-bird cambric, 75 cents net.

IT will come somewhat as a surprise to those of our readers who know Prof. Wood only as a physicist to learn that the present booklet contains nothing but quaint illustrations and jest in verse. The volume is obviously a satire directed against the sentimental nature-study literature which sometimes masquerades as scientific teaching, particularly in the United States.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

Winding of Rivers in Plains.

A CURIOUS obsession as to a matter of fact, to which everyone is more or less liable when obfuscated by an erroneous theory, has recently been noticed by me in some geological books, *e.g.* in Le Conte's "A Compend of Geology," and in Tyndall's "Glaciers of the Alps." I noticed it first in my late colleague Prof. Watts's recent little text-book of geology; but, indeed, I have not found any book of the kind quite clear and correct on the subject.

The statement is clearly made and illustrated by a figure that the flow of a bending river is most rapid on the outer side, where its banks are concave; and the well-known scouring or excavating action which a stream exerts on this bank is then attributed to this imaginary more rapid flow.

But the fact is that the flow is most rapid on the inner or sediment-depositing side of the bend, and Prof. James Thomson showed in 1876, in a well-known communication to the Glasgow meeting of the British Association—when he exhibited a model, confirming calculations previously made by himself—that the excavating action of a river is not due to the direct scouring action of the main stream at all. The explanation which he gave was virtually as follows :—

The rapid flow on the inner and strongly curved side of the bend piles up the water on the outer side by centrifugal force, so that near the concave bank it is nearly stationary, but elevated; its energy there is potential, not kinetic. Now if the rapidity of flow were uniform from top to bottom the slope would be in equilibrium; but owing to the retardation of the bed the flow near the bottom is slower, and there is not nearly so much centrifugal force exerted down below; wherefore the piled-up water is continuously returning from upper to lower level, that is, from the concave to the convex bank, as an undercurrent, almost at right angles to the main stream, bringing with it, by its undertow, silt and solid matter, which it deposits near the inner side of the bend, thus constantly increasing its own sinuosity in the well-known way.

The stream itself, combining a progressive with a lateral

circulating motion, may be said to *screw* itself like a corkscrew round a bend: and it is the lateral circulation which shifts the bed.

So much for streams, now for glaciers. Prof. Tyndall, as is well known, took careful measurements of the flow of glaciers, and finding that their line of quickest motion was *more* sinuous than the glacier bed, said that this was another analogy to the flow of a river.

There, however, he was in error. The line of most rapid flow of a river is *less* sinuous than the river itself. The water flows round the bend somewhat as it would flow in a vertical columnar vortex; most rapid on the inside, and almost stationary or even retrograding on the outside of some bends. If ice flows otherwise—and I have no reason whatever to doubt Tyndall's measurements—it must be because the rate of change of momentum of so slow a motion, compared with its lateral stiffness, is very small; so that we might certainly anticipate that the laws of its flow would be in many respects different from—though also in some respects singularly like—those of a liquid of but small viscosity. Probably it obeys exactly the laws of an *extremely* viscous liquid the viscosity of which could be specified. The flow cannot be much governed by inertia, as that of water is.

But I know that glacier motion is a thorny subject upon which I have no desire to tread. I would not be understood as making any assertion concerning it, but merely throw out a hint.

As to winding rivers, however, the matter is fairly simple; and the writers of geological and geographical text-books may easily amend some incautious though natural statements as to matters of fact, which they sometimes illustrate by erroneous diagrams.

OLIVER LODGE.

Recalculation of Atomic Weights.

DURING the last few years our knowledge of the accurate atomic weights of the elements silver, sodium, potassium, chlorine, and bromine has been greatly extended by the masterly researches of T. W. Richards and his colleagues. At present, however, there is no really trustworthy value for the ratio of silver to oxygen, and a satisfactory value for nitrogen has only just been obtained by Gray and by Guye.

While reading an account of the determination of the ratio $Ag : AgNO_3$, it occurred to me that this result, together with others previously obtained by Richards, afforded a means of calculating absolutely the atomic weights of the above-mentioned elements in terms of oxygen. The following values are available :—

				Error
(1)	$Ag : KCl = 100 : 69\cdot1073$	...	...	$0\cdot0004 = v$
(2)	$AgCl : KCl = 100 : 52\cdot0118$	...	...	$0\cdot0004 = x$
(3)	$Ag : AgNO_3 = 100 : 157\cdot479$	...	..	$0\cdot001 \ = w$
(4)	$Ag : AgCl = 100 : 32\cdot867$	...	...	$0\cdot0005 = y$
(5)	$N_2O_5 : K_2O = 100 : 87\cdot232$	...	...	$0\cdot002? = z$

We have thus five simultaneous equations, from four of which the four unknown quantities Ag, K, Cl, and N can be calculated in terms of O. Neglecting 2, and putting $O = 16$, I worked out the atomic weight of N, and was intensely surprised to find the value 13·940.

Now it is a well-known fact that the final results of an "indirect analysis" such as the above may be greatly influenced by a small experimental error, and so I proceeded to estimate the maximum effect which could thus be produced. Putting $(69\cdot1073 + v)Ag = 100$ KCl, where v is the error, instead of the first equation above, and similar expressions for the last three, I obtained the formula

$$N = \frac{2422\cdot08 + 600v - 600y - 336w - 287z}{2779\cdot94 - 200v + 200y + 174w + 115z} \times 16.$$

From this it is evident that, if v is made positive and the other three quantities negative, the numerator will be increased and the denominator diminished, both these facts tending to raise the value of N. Putting for the symbols their values given in the table of errors, the following result is obtained :—

$$N = \frac{2422\cdot08 + 1\cdot45}{2779\cdot94 - 0\cdot58} \times 16 = 13\cdot951.$$

In a precisely similar way, but using equation 2 instead of 1, the value 13·937 is obtained, which can be increased to 13·948.

This being the case, the question is, How can this value arise? The experimental work upon which the figures are based seems to have been carried out with every possible precaution, and all the values agreed very closely. The only possible weakness appears to lie in equation 5; the result was deduced from only three experiments, and the agreement was not so good as usual. Assuming for the moment that this value is wrong, it is easy to calculate by how much it is so. Taking N=14·010, we may say

$$N = 14\cdot010 = \frac{2422\cdot08 + 6\cdot62}{2779\cdot94 - 3\cdot57} \times 16.$$

If the values of x, w, and y are taken as before, this gives 0·028 as the *minimum* value of z, and it is hardly to be expected that such a large error could have escaped notice. Another possibility is that all the errors are about five times as large as the values given, but even if this were so it would be very improbable that they should all be of such a nature as to raise the atomic weight. Consequently, granted that the discrepancy is due to experimental error, it is almost certain that the analysis of potassium nitrate is at fault. This was carried out by heating with silica, and if the nitrate was not completely decomposed the number 87·232 would be too great. This is the only explanation which seems reasonable; and, moreover, if the ratio obtained in this experiment be changed to 100 : 87·203, all five equations become consistent, and yield frequently accepted values for the atomic weights, silver being 107·883.

Whether this be the true explanation or not, it is obvious that the method outlined above affords an excellent means of checking atomic weight determinations, and is also applicable to finding the absolute weights, since there is no great accumulation of errors.

                         H. E. WATSON.
University College, Gower Street, October 26.

The Fauna of Madagascar.

MADAGASCAR, with certain adjacent islands, has been regarded by some naturalists as forming a distinct " region," the Malagasy, equivalent to the other main regions of the world. On the other hand, Messrs. P. L. and W. L. Sclater ("The Geography of Mammals," London, 1899, p. 108) adhere to the earlier opinion of the first-named of the two authors, as well as of many subsequent writers, and place Madagascar in a subregion only of the Ethiopian region. They remark that "Madagascar appears to contain a sample of the ancient Ethiopian fauna, which has been almost exterminated on the mainland."

The archaic nature of the Madagascar fauna has lately (*Zool. Jahrb.*, 1902) received further confirmation at the hands of Miss A. Carlsson, who found that the peculiar Viverrid genus Eupleres showed likenesses to both the Viverrine and Herpestine sections of the Viverridæ, and was therefore probably an ancient type of Viverrid. Having had lately the opportunity of making some anatomical observations upon another Madagascar genus, viz. Galidictis, I am able still further to support this view. This interesting Viverrid has the external scent glands of the Viverrine section, and a cæcum which is comparatively long, like that of the Herpestinæ. The brain, like that of Eupleres, shows intermediate characters. Finally, the archaic nature of this animal is demonstrated by the completely double uterus, a feature new to the Carnivora, where a bicornuate uterus is at least the rule.

It has been pointed out that Madagascar also shows an unexpected likeness to the neotropical region in its fauna, especially in the group of reptiles. As to mammals, the late Dr. Dobson showed reasons for believing that the alleged close resemblance between the Cuban Soleonodon and the Mascarene Centetes had been exaggerated; but among the Reptilia there are genera which are common to the two regions, *e.g.* the snakes Boa and Corallus. I have been able lately to compare *Corallus madagascariensis* with a South American form, *C. cookii*. In the former the bronchus extends a long

way down the larger lung, the liver is prolonged by one lobe nearly to the gall bladder, the umbilical vein of the fœtus does not persist, and the mode of distribution of the intercostal arteries is as in the pythons. In the latter species these characters are as in the Anaconda.

The anatomical differences may possibly seem slight to those not specially acquainted with the structure of serpents; but in the features mentioned there is, if anything, rather a greater difference between the two species of Corallus than between two admittedly distinct genera such as Eryx and Python. It is very desirable that the alleged close resemblance between other forms occurring both in Madagascar and in the neotropical region should be carefully scrutinised.   FRANK E. BEDDARD.
     Zoological Society's Gardens.

The Interpretation of Mendelian Phenomena.

DR. ARCHDALL REID's letter in NATURE of October 3 contains a very positive statement in reference to the relation of Mendelian phenomena to man, which I think should be immediately answered. I delayed supplying an answer because I wished to discuss his statement on a tangible basis. I desired to analyse certain data which I have been collecting, and which throw light upon the problem of segregation in man. This analysis is as yet incomplete, but it is sufficient to show that Dr. Archdall Reid is too confident when he asserts that "there is no segregation in man," and that, "with the exception of eye-colour, and possibly one or two other traits, such as the Mongolian eyelid, human hybrids appear to blend every character as perfectly as skin-colour."

The accounts which I have collected deal with various marriages between Europeans (chiefly Scotch) and the Canadian Red Indians. It is well known that many of the early European settlers in Canada married Red Indian women. The resulting half-breeds in their turn were in some cases intermarried, and in others mated to Europeans.

The Canadian Red Indians can be marked off from Europeans by six definite characters, which concern the nature of the hair, eyes, skin, cheek-bones, nose, and beard. The Indian hair is invariably black, long, glossy, and lank, and cannot be confounded with European hair; the eyes are almost invariably black or, very seldom, dark brown; the skin is tawny brown-yellow (varying from pale olive-yellow to dark brownish yellow); the cheek-bones are high (there is no obliquity to the eyes, thus differing from the Mongol); the nose is very prominent and broad at the base, and is of the *busque* type, that is, the profile is made up of two lines, which diverge widely from the bridge towards the base; and, lastly, there is either no beard or a very scant one of straight hairs on the face of the men. These characters, when well developed, are so different from the corresponding features in Europeans that they cannot easily be confused. No one, for instance, would mistake the long, lank, black hair and black eyes of the Indian for the thick red hair and blue eyes of some of the Scotch persons concerned in the histories now under review.

We may therefore use these six characters as differentiating ones, and may tentatively regard the Indian characters as being allelomorphic to the corresponding European ones. For the sake of brevity I will use symbols, which will have the following significance :—
I = Indian,   E = European,   H = hair,   E = eyes,   S = skin, C = cheek-bones,   N = nose.

First, then, with regard to the matter of dominance. We must, in this case, be quite sure that the European concerned marries a full-blood Indian. In the cases which I have so far collected, I have only one marriage of such an Indian with a European, and there were only two children of the marriage. The European was a Highland Scot. His complexion was fair, and eyes blue. I have no information of the colour of his hair, since it was white with age when observed, but it was quite thick and not lank. In all the features (with the exception of the beard, of which I have no information) which mark off the Indian from the European, the son and daughter of this marriage were quite Indian.

So far, then, as this one case will take us, these five